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COORDINATION RISK AND THE ROLE OF
COORDINATION STOCK

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ORDER STABILITY IN SUPPLY CHAINS: COORDINATION RISK AND THE ROLE OF COORDINATION STOCK

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Abstract

The *bullwhip effect* describes the tendency for the variance of orders in supply chains to increase as one moves upstream from consumer demand. Previous research attributes this phenomenon to both operational and behavioral causes. Operational causes are features of the institutional setting that lead rational agents to amplify changes in demand, while behavioral causes arise from suboptimal decision-making. This paper examines causes of the bullwhip through experiments with a serial supply chain, using the Beer Distribution Game. Unlike prior studies, we control all four commonly cited operational causes of the bullwhip, including uncertainty about customer demand. We eliminate demand uncertainty completely by making customer demand constant and known to all participants. Despite these controls, order amplification, instability, and supply line underweighting remain pervasive. We propose a new behavioral cause of the bullwhip, *coordination risk*, that arises when players place excessive orders to address the perceived risk that others will not behave optimally. We test two strategies to mitigate coordination risk: (1) holding additional on-hand inventory, and (2) creating common knowledge by informing participants of the optimal policy. Both strategies reduce, but not eliminate, the bullwhip effect. Holding excess inventory reduces order amplification by providing a buffer against the endogenous risk of coordination failure. Such *coordination stock* differs from traditional safety stock, which buffers against exogenous demand uncertainty. Surprisingly, neither strategy reduces supply-line underweighting. We conclude that the bullwhip can be mitigated but its behavioral causes appear robust.

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1 Introduction

Supply chain instability is costly because it creates excessive inventories, poor customer service, and unnecessary capital investment. Despite the undoubted benefits of the lean manufacturing and supply chain revolutions of the past decade, supply chain instability continues to plague many businesses. Examples include excess capacity, price cuts, layoffs, and record inventory write-offs in computers, semiconductors, telecommunications, and other high-tech industries since 2000, and waves of zero-interest financing and cash-back incentives accompanying surplus inventory in the automobile market. The *bullwhip effect* is a major source of inefficiency in product supply chains. The bullwhip effect refers to the tendency for order variability to increase at each level of a supply chain as one moves from customer sales to production (see Lee et al. 1997 and Sterman 2000 for industry examples).

Previous research offers two categories of explanation for the bullwhip effect, each motivating distinct recommendations to dampen it. The first category focuses on *operational* causes of the problem, such as errors in demand forecasting, order batching, gaming due to perceived or real shortages, and price fluctuations caused by promotions (Lee et al. 1997). These causes have been documented in practice, and techniques to eliminate them are now an important part of the tool kit for supply chain design (e.g., Simchi-Levi et al. 1999).

The second category, first introduced by Forrester (1958, 1961) and further developed by Sterman (1989a, 1989b), focuses on *behavioral* causes of instability. The behavioral explanation emphasizes the bounded rationality of decision makers, particularly the failure to adequately account for feedback effects and time delays. Research into the behavioral causes of the bullwhip effect includes experimental studies of decision making in supply chains with order processing and fulfillment delays. These studies consistently show that people do not adequately account for the time delays, feedbacks, and nonlinearities in the system. Specifically, people tend to place orders based on the gap between the target level of inventory and their current, on-hand stock, while giving insufficient weight to the supply line of unfilled orders (the stock of orders placed but not yet received). Supply line underweighting is sufficient to cause the instability observed in both experimental and real supply chains (Sterman 1989a, Sterman 2000).

Supply chain performance reflects both operational and behavioral factors. Operational factors—the physical and institutional structure, including information availability—determine the potential performance of the system, that is, the performance achieved by rational agents. A supply chain with short delays between placing orders and deliveries will behave better than one with long delays; a system with stable demand will have lower costs and less order variability than one with large unanticipated changes in customer orders; when consumer demand is publicly known performance will be better than

when demand data are unavailable. The second determinant of performance is the quality of the decision rules used by the agents. If the system is disturbed from equilibrium, rational agents will place orders that return the system to equilibrium in the least-cost fashion, without oscillation or instability. Order amplification will be as small as possible given the institutional structure. Agents using suboptimal decision rules based on inappropriate mental models of the task, however, may cause the closed-loop response of the supply chain to be underdamped or even locally unstable. For any latent instability to manifest, however, there must be some initial disturbance to knock the system out of equilibrium. In assessing supply chain performance it is necessary to distinguish between the stability of the system's response to perturbations and the sources of perturbations that trigger its disequilibrium response.

Perturbations can be exogenous, for example, unanticipated changes in customer demand. But there can also be endogenous perturbations. For example, managers who suspect their customers or suppliers will make poor decisions may choose to deviate from the equilibrium strategy to build a buffer stock against the risk of non-optimal behavior, thus causing variability in the orders they impose on their suppliers. We term the risk associated with uncertainty about the actions of others *coordination risk*. Such deviations may serve to trigger latent instability caused by supply line underweighting.

Prior work demonstrates that the tendency to underweight the supply line is sufficient to account for the bullwhip effect. Here we examine the extent to which coordination risk causes decision-makers to deviate from optimal behavior, thus triggering latent instability caused by supply line underweighting. We further test two mechanisms to mitigate coordination risk: (1) adding additional inventory to buffer against variability arising from coordination failure, and (2) creating common knowledge about the optimal ordering policy, to reduce the incidence and magnitude of coordination failure and suboptimal decision making. We test for the existence of coordination failure, as well as the effectiveness of these two counteracting mechanisms, through four controlled experiments using the beer distribution game.

The beer distribution game is well suited to examine these issues. It is simple enough for people to learn while retaining key features of real supply chains. Each stage of the simulated supply chain includes on-hand and on-order inventory, delays in order fulfillment, and backlogging of unfilled orders. As in real supply chains, the order fulfillment time is endogenous. When a person's immediate supplier is in stock, the fulfillment lead-time is fixed and short. However, if the supplier stocks out, incoming orders accumulate in the supplier's backlog until sufficient stock can be acquired to fill them, causing the order fulfillment time to increase. Thomsen et al. (1992), Mosekilde (1996), and Larsen et al. (1999) show that this nonlinearity strongly conditions the stability of the system.

To provide the most difficult test of the behavioral theories of supply chain instability our

experiments eliminate the four commonly cited operational causes of the bullwhip effect: order batching, gaming due to shortages, price fluctuations caused by promotions, and demand signaling due to forecast uncertainty. Earlier work using the beer game (e.g. Sterman 1989a) eliminated the first three causes.¹ However, the traditional game protocol does not eliminate demand signaling since the demand forecast is unknown to participants. Croson and Donohue (2002) used a stationary distribution for customer demand and provided participants common information about that distribution (though not of its realizations). Results under these conditions were similar to those in Sterman (1989a). Here we eliminate demand variability altogether by using constant demand of 4 cases per week, which is publicly announced to all participants. With constant, known demand, no exogenous disturbances of any type, and all operational sources of order amplification removed, rational agents should not deviate from ordering 4 each period, resulting in no bullwhip effect. However, contrary to expectations based on assumptions of rationality, we find that the bullwhip effect and oscillations persist.

Econometric estimation of individual participants' decision rules shows the vast majority significantly underweight the supply line of unfilled orders, consistent with prior studies. Post-play questionnaires and participant debriefing further suggest many players were unable to predict how their teammates would behave, violating the common knowledge assumption of game theory (the assumption that each player knows others will play their best response in any situation so that their behavior can be predicted). The inability of players to anticipate the orders placed by their teammates creates coordination risk that can serve as a trigger for the instability, amplification, and oscillation caused by supply-line underweighting.

Experiments 2 and 3 provide suggestions of how to address coordination risk. Experiment 2 shows that holding additional inventory is beneficial in the presence of coordination risk as it buffers the system against players' ordering mistakes. The results suggest a new purpose for inventory, which we term *coordination stock*. Coordination stock differs from conventional safety stock, inventory held to buffer against exogenous uncertainty in demand or deliveries. In contrast, coordination stock is inventory held to buffer against endogenous uncertainty about the actions of other supply chain members. We find that modest coordination stocks significantly lower order variability and costs, suggesting a new purpose for "excess" inventory in decentralized supply chains.

¹The standard protocol for the beer game eliminates the need for order batching because there are no fixed ordering costs or quantity discounts. Each player orders from and ships to only one other and production capacity is infinite, eliminating the incentive for gaming in response to shortages. Finally, prices are fixed and customer demand is exogenous, eliminating the possibility of promotions and forward buying.

Coordination stock improves performance by keeping the system farther from the unstable regime in which stockouts lengthen delivery times, intensifying the fluctuations caused by supply-line underweighting. Coordination stock addresses the symptoms but not the root cause of the bullwhip. In experiment 3 we examine the effect of reducing coordination risk by creating “common knowledge” about the optimal ordering policy. Under constant, known demand the optimal ordering policy is a simple order-up-to policy that fully accounts for both on-hand and on-order inventory. We repeat the conditions of experiment 1 except that all participants are publicly informed of the optimal ordering rule. Performance should improve for two reasons. First, the tendency to underweight the supply line should disappear, or at least decline substantially. More subtly, publicly providing the optimal rule creates common knowledge among the players, and should eliminate (or at least reduce) coordination failure. Publicly providing the optimal rule should both increase the stability of the system and reduce the incidence of perturbations that might trigger instability. However, we find that amplification, while reduced, is still significant, and most participants continue to underweight the supply line despite knowledge of the optimal policy.

Providing the optimal policy to participants does not guarantee that they will implement it correctly, so some coordination risk may remain. Experiment 4 eliminates coordination risk completely by automating the decisions of three of the four supply chain members. The single human decision maker is informed of the optimal decision rule and that the automated supply chain members are programmed to follow it. We find that order variability generated by the human player decreases significantly relative to experiment 1 (no common knowledge of the optimal rule) but not compared to experiment 3 (where common knowledge of the optimal rule is provided but its use is not guaranteed). Estimation of the human players’ decision rules shows no moderation in the tendency to underweight the supply line. The improvement appears to be due to the reduction in the variance of the environment caused by replacing other humans with optimal agents, not improvement in the humans’ decision rules.

Overall, our results show a significant role for behavioral causes of the bullwhip effect. Coordination stock reduces the impact of poor decision rules, and providing common knowledge of the optimal policy improves performance. Yet the bullwhip remains. Supply line underweighting is highly robust—it does not vary significantly by supply chain role and is not significantly moderated by any of our treatments.

In the next section we present the details of our experimental design. We report the results of the four experiments in sections 3 through 6 and show that the well-established tendency to under weight the supply line is not affected by our coordination-related controls. Section 7 summarizes the results and associated managerial implications.

2 Experimental Design and Implementation

Our experiments follow the standard protocol of the Beer Distribution Game. The game consists of four agents (retailer, wholesaler, distributor and manufacturer/factory), each making ordering decisions for one link in a serial supply chain with exogenous final customer demand (Figure 1).

[Insert Figure 1 about here]

The chain operates as a multi-echelon inventory system in discrete time with demand backlogging, infinite capacity, and both order processing and shipping lags (see Chen 1999 and Clark and Scarf 1960 for classic models of such systems). Each period (corresponding to one week) all players experience the following sequence of events: (1) incoming shipments from the upstream decision-maker are received and placed in inventory, (2) incoming orders are received from the downstream decision-maker and either filled (if inventory is available) or placed in backlog, and (3) a new order is placed and passed to the upstream player. Croson and Donohue (2002) and Stermann (1989a) provide further details on the structure and sequence of play.

As in most previous studies, the retailer, wholesaler, and distributor (R, W, D) face a two week lag between placing an order and fulfillment by their immediate supplier, and an additional two week transportation lag between fulfillment by the supplier and delivery (a total lag of 4 weeks, assuming the supplier is in stock). The manufacturer/factory (F) has a one-week lag between placing orders (setting the production schedule) and production starts, and a two-week production lag (a total lag of 3 weeks). As in earlier studies, costs are \$0.50/week for each unit held in inventory, and \$1/week for each unit backlogged.

The major difference between our study and prior work is the treatment of demand. In previous work customer demand both varied across time periods (i.e., was either stochastic or non-stationary), and was unknown to participants. For example, Stermann (1989a) and Steckel et al. (2004) use unknown deterministic demand functions (a step function and S-curve, respectively). Croson and Donohue (2002, 2003) use a known, stationary uniform distribution, but agents (other than the retailer) do not know the realizations of demand. Due to the order fulfillment delay, agents must forecast future demand. It is conceivable that the bullwhip in these settings results, in part, from demand uncertainty and resulting forecasting errors (Chen et al. 2000). In our study, we eliminate demand variability and forecasting as potential causes of the bullwhip by using a constant demand of 4 cases per period and informing participants of this fact before the game begins. A manipulation check confirms that participants understand these facts before play starts. Further, the realization of demand each week (4 cases) is displayed on every agent's screen at all times, confirming visually that demand is indeed constant. All

experiments begin in flow equilibrium with orders and shipments of four cases at each delay step. Initial on-hand inventory levels vary across treatments to explore the impact of coordination stock.

The experiments were run using a computer network with an interface written in Visual Basic. The complete set of instructions are available in the on-line supplement. All sessions were conducted at the Penn State Smeal College of Business Laboratory for Economic Management and Auctions (LEMA) in the spring semester of 2003. A total of 240 participants were recruited using an on-line recruitment system. Most were students, primarily undergraduates, from various fields of study including business and economics. Participants were paid a \$5 show-up fee, plus up to \$20 in bonus money depending on their team's relative performance. Team performance was computed based on total supply chain cost (i.e., the cumulative sum of holding and backorder costs across all four players) using the continuous payment introduced by Croson and Donohue (2002), and further described in Appendix A-1. After the rules were explained, but before play began, participants were given a quiz to confirm they understood the rules and calculations, as well as the fact that customer demand would be constant at 4 cases/week for the entire game (see on-line supplement for a copy of the quiz). After the game, but before receiving payment, participants were asked to complete an on-line questionnaire inviting them to reflect on their experience.

We tested the resulting data for outliers using the Grubbs procedure (Grubbs 1969; also see Appendix A-2 for more information). A small number of outliers were found and deleted from the statistical analyses reported in the main text. Eliminating the outliers reduces the between-condition variance in the different treatments, an *a fortiori* procedure that provides a tougher test of our hypotheses. For completeness, we also report results using the entire data set (i.e., including outliers) in footnotes. All our findings hold at similar levels of significance with or without outliers.

3 Experiment 1: Constant Demand

When demand is known and constant there is no need for safety stock, so optimal on-hand inventory is zero for all supply chain levels. Experiment 1 examines how participants behave in this equilibrium setting, with initial on-hand inventory set to the minimum cost level of zero for all four players.

3.1 Theoretical Predictions

Because the system is initialized in flow equilibrium it is trivial to show that decision makers minimize costs by simply passing through constant orders of four. In our setup, if players understand the optimal policy and believe that the other players also understand the optimal policy (i.e., if the common knowledge assumption holds) players should all order 4 units each week, ensuring that all inventories remain constant at the optimal level of zero. The system remains in equilibrium and there will be no

order oscillation or amplification (i.e., no bullwhip). Hence:

Hypothesis 1: *The bullwhip effect (order oscillation and amplification) will not occur when demand is known and constant and the system begins in equilibrium.*

3.2 Experimental Results

Figure 2 shows orders and on-hand inventory over time for each individual in the ten teams in experiment 1. The characteristic pattern of oscillation and amplification observed in earlier experiments persists despite constant, known demand. None of the teams remains close to the equilibrium throughput of 4 cases/week. Orders at the factory peak at an average of 4291 cases per week, with a range of 24 to 40,000. Teams 4 and 9 reach peaks of 40,000 and 1,183 respectively and are excluded from subsequent analysis as outliers (see Appendix A-2). Excluding these outliers the mean peak factory order is 215 (the maximum peak order is 500, and the average factory order is 19). Figure 3 displays the standard deviations of orders for each role across the eight teams included in our data analysis.

[Insert Figures 2, 3 about here]

Significant oscillations occur even with constant demand. We use a non-parametric sign to test for order amplification (see Seigel (1965), p. 68). Order amplification exists when the standard deviation of the i^{th} stage in the supply chain, σ_i , exceeds that of its immediate customer, σ_{i-1} (where $i \in \{R, W, D, F\}$). If there were no bullwhip, we would observe $\sigma_i > \sigma_{i-1}$ at the chance rate of 50%. The data reveal $\sigma_i > \sigma_{i-1}$ for 79% of the cases, rejecting H1 at $p = 0.0025$.² The bullwhip effect persists even when customer demand is known and constant.³

3.3 Individual Ordering Behavior

Experiment 1 demonstrates that the bullwhip effect is highly robust in the face of experimental manipulations that should eliminate it. To gain insight into the decision processes of the participants we specify a decision rule for managing inventory in the simulated supply chain and then estimate its

² Including the outlier teams 4 and 9, $\sigma_i > \sigma_{i-1}$ for 80% of the cases, and H1 is rejected at $p = 0.0005$.

³ To test the robustness of our results to the use of the standard deviations of orders as the metric for amplification we also analyzed the root mean squared (RMS) deviation, δ , of orders, O , from the steady-state optimal value of 4, $\delta_{i,j} = (1/n)[\sum_{t \in \{1, 48\}} (O_{i,j,t} - 4)^2]^{1/2}$ for role i in team j . The RMS deviation penalizes participants for all deviations from optimal, and not only for variability around the participant's mean orders. All statistical results reported here continue to hold using the RMS deviation from optimal instead of the standard deviation, and hence are not described here, but can be found in Table 2.

parameters for each participant. Comparing the estimated decision weights to the optimal weights gives insight into the performance of participants.

Prior studies show that people have great difficulty managing complex dynamic systems, typically failing to account for feedback processes, underweighting time delays, and misunderstanding stocks and flows (Sterman 1994, Booth Sweeney and Sterman 2000). Underweighting the supply line of unfilled orders in stock management tasks such as the beer game is particularly common. Sterman (1989a) showed that participants in the beer game significantly underweighted the supply line, causing much of the instability and amplification observed, a result replicated by Croson and Donohue (2002, 2003). Sterman (1989b) and Diehl and Sterman (1995) show underweighting is robust in one-person tasks where by definition there are no strategic interactions among players and hence no issues of trust in others' actions or coordination failure. Studies showing that time delays reduce performance relative to potential and slow learning in other dynamic tasks include Brehmer (1992), Dörner (1980, 1996), Kleinmuntz and Thomas (1987), Kleinmuntz (1993), and Kampmann and Sterman (1998).

Following Sterman (1989a) we estimate the following decision rule for orders $O_{i,t}$ placed in week t by the person in role i :

$$O_{i,t} = \text{Max}\{0, CO_{i,t} + \alpha_i(S'_i - S_{i,t} - \beta_i SL_{i,t}) + \varepsilon_{i,t}\} \quad (1)$$

where CO is expected Customer Orders (orders expected from the participant's customer next period), S' is desired inventory, S is actual on-hand inventory (net of any backlog), and SL is the supply line of unfilled orders (on-order inventory). Orders are modeled as replacement of (expected) incoming orders modified by an adjustment to bring inventory in line with the target.

The parameter α is the fraction of the inventory shortfall or surplus ordered each week. The parameter β is the fraction of the supply line the participant considers. The optimal value of β is 1, since participants should include on-order as well as on-hand inventory when assessing their net inventory position. The optimal value of α is also 1 since participants should order the entire inventory shortfall each period. The optimal expectation for customer orders in our experiment where customer demand is stationary (indeed, constant and known to all participants) is the mean of actual orders placed by the final customer, that is, $CO_{i,j} = 4$ cases/week for all sectors i). In this case the rule reduces to the familiar order-up-to rule. The parameter S' represents the sum of the desired on-hand and desired on-order inventory. Since final customer demand is constant and known, desired on-hand inventory is zero, and desired on-order inventory is the inventory level required to ensure deliveries of 4 cases/week given the order fulfillment lead time of 4 weeks (3 for the factory), yielding $S' = 16$ units (12 for the factory).

Equation (1) can also be interpreted as a behavioral decision rule based on the anchoring and

adjustment heuristic, in which expected customer orders $CO_{i,j}$ represents the anchor (order what you expect your customer to order from you), and inventory imbalances motivate adjustments above or below the forecast. To capture the possibility that participants do not use the optimal forecast of incoming orders (the constant rate of 4 cases/week), but rather respond to the actual orders they receive, we model expected customer orders as formed by exponential smoothing, with adjustment parameter θ_i , as in Sterman (1989a),

$$CO_{i,t} = \theta_i IO_{i,t-1} + (1 - \theta_i) CO_{i,t-1} \quad (2)$$

where IO is actual Incoming Orders to each position.

We use nonlinear least squares to find the maximum likelihood estimates of the parameters θ_i , α_i , β_i , and S'_i , subject to the constraints $0 \leq \theta_i, \alpha_i, \beta_i \leq 1$ and $S' \geq 0$. We assume an iid Gaussian error ε with variance given by the variance of the observed residuals for each individual, σ_i^2 . We estimated the confidence intervals around each estimate with the parametric bootstrap method widely used in time series models of this type (Efron and Tibshirani 1986, Li and Maddala 1996, Fair 2003). An ensemble of 500 bootstrap simulations were computed for each participant, and the 95% confidence intervals for each of the four estimated parameters computed using the percentile method. Table 1 summarizes the results.

[Insert Table 1 around here]

As expected, the uncertainty bounds around θ are large. For many participants the variance in incoming orders is small, and sometimes zero, e.g., when their customer always orders the optimal quantity of 4 cases/week. In such cases the smoothing time constant cannot be identified. More important are the estimates of α and β . In experiment 1, the median fraction of the inventory shortfall corrected each period is 0.43, and the median fraction of the supply line participants consider is 0.10, both far below the optimal values of 1. The estimates of α and β are generally tight, with the median width of the 95% confidence band equal to 0.32 and 0.34, respectively. About 98% of the estimated values of α are significantly greater than 0 (most participants respond to inventory imbalances, as expected), but 73% are significantly less than the optimal value of 1 (most participants do not use the optimal order-up-to rule but instead order only a fraction of their inventory shortfall each period). For β , 78% of the estimates are significantly less than the optimal value of one. Further, 59% are not significantly different from zero, and the best estimate of β is zero for 22% of participants (participants tend to ignore on-order inventory). Participants' post-play questionnaire responses are consistent with supply line underweighting, e.g.,

“I watched what the others downstream were ordering, but I completely forgot to factor in my lag time for filling orders for MY inventory!” [Factory]

“The timelag in orders was a bit difficult to anti[ci]pate” [Distributor]

“I decided how much to order based on the backlog that I had for the prior week. If I was going deep into the hole I would order even more from the distributor” [Wholesaler].

3.4 Discussion

Contrary to predictions based on reasonable assumptions of rationality and common knowledge, the bullwhip effect remains even when demand is constant and known. The fluctuations and amplification arise because most participants do not account adequately for the supply line of unfilled orders. Faced with an inventory shortfall, they order enough to correct the imbalance, but, since these orders do not arrive for some weeks, on-hand inventory remains inadequate and they order the required quantity again and again, ensuring that on-hand inventory overshoots the desired level. The failure to account for the supply line is responsible for the oscillation observed in the trials, and for most of the amplification in order rates from retailer to factory. Further evidence of supply line underweighting is found in estimates of desired total inventory S' , which are far too small to account for the replenishment lead time. The median estimate of S' is 4.7 cases, while the optimal value is at least 16 (12 for the factory).⁴

As shown in Table 1, the estimated parameters are close to the values found in Sterman (1989a) and Croson and Donohue (2002). Participants substantially underweight the supply line and underestimate the length of the replenishment cycle time. These decision rules ensure that any disturbance that moves the system out of equilibrium will trigger oscillations and lead to the bullwhip, even though final demand is constant and known.

Failure to consider the supply line explains why the system oscillates, and why orders are amplified, given that at least one participant in the supply chain deviates from equilibrium. However, if all participants ordered four cases/week, on-hand and on-order inventories would always remain at the optimal levels and there would be no supply line imbalance to underweight. There must be some initial deviation from equilibrium to elicit the latent instability. In Sterman (1989a) the system is knocked out of equilibrium by the step increase in customer orders. In Croson and Donohue (2002) the system is perturbed by random variations in demand. What is the trigger when demand is constant and known, and the system is initialized with the optimal inventory level of zero? Initial deviations from optimal orders

⁴The optimal value of S' of 16 (12 for the factory) assumes subjects seek the optimal level of 0 on hand inventory. Those who fear their teammates may deviate from optimal may seek to hold coordination stock > 0 . If so, the imputed values of desired on-order inventory would be even smaller and less adequate.

could arise for a variety of reasons, including cognitive errors or inaccurate mental models. Some of the comments in our post-play questionnaire suggest another factor. A number of participants asserted that while *they* realized that ordering 4 units every period would minimize costs in this environment, they were unsure whether their *counterparts* understood and would follow the optimal policy:

“Ordered 4 each week until the manufacturer started sending me odd quantities.” [Retailer]

“Honestly, I could not see any strategy at all in what my customer ordered.” [Wholesaler]

“I think the customer was making unreasonable demands and panicking causing everyone else to do so also, so I could never be where I wanted to be.” [Distributor]

“I think the wholesaler had no strategy and was simply experimenting.” [Factory]

While such comments may represent *ex post* self-justification by blaming others for their poor performance, *ex ante* uncertainty about the behavior of others might motivate some participants to hold additional inventory as a buffer against the risk of errors by their partners. To increase on-hand inventory from the initial level of zero requires a participant to temporarily increase orders, thus forcing the supplier into an unanticipated backlog situation and, to the extent the upstream players underweight the supply line, triggering the oscillations observed in Figure 2. Indeed, the data reveal that at least one participant in every team ordered more than 4 units in the first period. While supply-line underweighting is the root cause of the oscillations and amplification, coordination failure may serve as an important trigger.

Coordination risk can arise from a failure of common knowledge or a lack of trust in others’ actions. Failure of common knowledge occurs when individuals are not sure that others know the optimal policy. Trust breaks down when people are not sure others will follow the optimal policy, even when that policy is commonly known. Next we test whether holding additional inventory as a buffer against the risk of coordination failure reduces the oscillations and amplification generated by supply line underweighting.

4 Experiment 2: Adding Coordination Stock

In experiment 2 we test whether additional on-hand inventory mitigates the impact of coordination risk. Such a stabilizing benefit has been suggested by others (e.g., Cohen and Baganha 1998), but never tested in an experimental setting. Conditions are identical to experiment 1 except that all participants begin with 12 units of on-hand inventory rather than the optimal level of zero. We chose 12 units for two reasons. First, it is the initial inventory traditionally used in the beer game, facilitating comparison to prior work. Second, 12 units is likely to exceed the desired coordination stock, suggesting initial orders would fall rather than rise as in Experiment 1, a condition that should promote stability by preventing

backlogs from accumulating up the chain.

4.1 Theoretical Predictions

Additional on-hand inventory may improve performance for two reasons. First, if players judge that there is risk of coordination failure, they may seek to hold some buffer inventory. Initializing the experiment with positive on-hand inventory may then reduce the need to increase orders above the equilibrium throughput, reducing the incidence and magnitude of deviations that may trigger the oscillations and instability caused by supply line underweighting. Second, initial on-hand inventory reduces the likelihood that the system will enter the unstable backlog regime, thus reducing the magnitude of the oscillations generated by players who underweight the supply line. Hence,

Hypothesis 2: *Excess initial inventory will decrease order variability.*

4.2 Experimental Results

Figure 4 displays the average order standard deviation across all teams for experiment 2.

[Insert Figure 4 about here]

Order oscillation and amplification clearly remain, although the addition of coordination stock dampens them compared to the base-line setting (i.e., comparing Figures 3 and 4). The sign test suggests that the order amplification still exists in experiment 2, but it is only marginally significant ($\sigma_i > \sigma_{i-1}$ in 62% of the cases, differing from the chance rate of 50% at $p = 0.097$).⁵

To test the impact of coordination stock on order oscillation, we use a nonparametric two-tailed Mann-Whitney U test (also referred to as a Wilcoxon test, see Siegel (1965), pg. 116) to compare the standard deviations of orders in experiments 1 and 2. This test confirms the visual impression that the standard deviation of orders is significantly less when coordination stock is present. The median standard deviation of orders placed in experiment 2 is 3.8 compared with 16.6 in experiment 1, a significant difference ($p=0.012$). Our results support H2.

While coordination stock reduces order oscillation, one could argue that the improvement comes at the expense of increased costs from holding larger inventories. To test this effect, we compare average costs, by role, across the two experiments, see Figure 5.

[Insert Figure 5 about here]

The median total supply chain cost without coordination stock of 9,151 is significantly higher than the

⁵In Experiment 2 teams 6, 7 and 10 were removed as outliers. Including them yields $\sigma_i > \sigma_{i-1}$ in 63% of the cases, and the p-value falls to 0.051.

median total cost of 1,890 with coordination stock (Wilcoxon test, $p = 0.021$).⁶ These results should be interpreted with caution, however, since they depend on the assumed cost structure. With unit backlog cost twice the unit holding cost uncertainty means one is better off keeping a few units of inventory on-hand. The asymmetry in costs approximates the situation in many industries where stockouts not only lead to lost sales but erode a firm's reputation as a reliable supplier, potentially leading to loss of market share, lower prices, and other costs. However, if holding costs exceed stockout costs, holding excess inventory may be prohibitively expensive even if it reduces supply chain instability. Such a situation may exist for perishable and customized goods, products with high embodied value-added, product variants ordered infrequently, and other settings where firms tend toward make-to-order strategies.

Examining the estimates of α and β in experiment 2, the median fraction of the inventory shortfall corrected each period is 0.26, and the median fraction of the supply line participants consider is 0.26, both substantially below the optimal values of 1. In experiment 2, 78% of the estimates of β are significantly less than the optimal value of one, 44% are not significantly different from zero and the best estimate is zero for 19% of participants. The estimates of α and β in experiments 1 and 2 are not statistically different (2-tailed Wilcoxon test; $p = 0.07$ for α and 0.21 for β). Supply line underweighting persists in this second experiment. However the *consequences* of underweighting are lessened considerably by the addition of coordination stock.

4.3 Discussion

The addition of extra on-hand inventory helps to alleviate the costs of coordination failure. Even if some supply chain members were committed to following the optimal policy, players can reduce the costs of their counterparts' deviations by holding excess inventory. The additional stock reduces the likelihood that the system will enter the unstable regime in which suppliers stock out, backlogs accumulate, and the replenishment lead-time lengthens. Such events in real supply chains often lead to instability and higher costs through allocation gaming, phantom orders, expediting, the use of premium freight/LTL transport, and other reactions (Lee et al. 1997, Simchi-Levi et al. 1999, Sterman 2000, ch. 18.3).

Managers and researchers recognize inventory may be held for different reasons. For example, inventory that exists because of the need to batch orders to amortize fixed ordering costs is commonly

⁶ With the outlier teams the median standard deviation of orders in experiment 1 is 25.7 and for experiment 2 it is 5.0, which are significantly different at $p = 0.045$. The median total supply chain cost with outliers is 13,105 without coordination stock and 2,840 with coordination stock, and this difference is weakly significant ($p = 0.07$). H2 is supported with and without outliers.

referred to as *cycle stock*. Inventory used to buffer the effects of process uncertainty (to prevent starvation between work cells) is known as *buffer inventory*. *Safety stock* usually refers to inventory used to buffer against the impact of demand uncertainty. While coordination failure is a contributing factor to demand uncertainty, most inventory models view demand uncertainty (and thus safety stock) as existing due to known demand forecast error or natural (exogenous) demand variation. In contrast, the uncertainty we observe in these experiments does not arise from the external environment—it is self-inflicted. We introduce the term *coordination stock* to represent inventory used to buffer against endogenous uncertainty in orders or delivery due to coordination risk. Coordination stock buffers against decision errors, but does not reduce those errors directly. It may be that some fraction of what is considered “excess” on-hand inventory in supply chains may actually be serving this purpose. This is an interesting empirical question for future study.

5 Experiment 3: Creating Common Knowledge

We now test a mechanism to alleviate coordination risk and supply line underweighting by providing common knowledge about the optimal ordering policy. The problem of common knowledge has been studied in economics and game theory, both theoretically (see the review by Geanakoplos 1992) and experimentally (e.g. Lei, Noussair, and Plott 2001, Nagel 1995). Failures of common knowledge have been shown to lead to coordination failures and inefficient outcomes in a variety of settings, although we are the first to examine its impact on supply chains. Experiment 3 is identical to experiment 1 (the game begins in equilibrium with on-hand inventory of 0 at all four levels), but the sessions begin with the following public explanation of the optimal policy:

“The total team cost can be minimized if all team members place orders so as to make the total of their on-hand inventory and outstanding orders equal to a pre-specified target level. This target level is 16 for the retailer, wholesaler and distributor, and 12 for the manufacturer. This means that if the total on-hand inventory and outstanding orders is greater than or equal to the target level, the order that minimizes team cost is 0. But if this total inventory is less than the target level, the cost-minimizing order is to order just enough to bring it to the target.”

The explanation was presented in written instructions as well as explained publicly along with the rest of the game rules. The explanation places no restrictions on participants’ actions, but the fact that the explanation is public provides common knowledge—each supply chain member knows the cost minimizing policy and knows that every other player knows it.

5.1 Theoretical Predictions

If our explanation of the cause of bullwhip behavior is correct, then reducing the likelihood of coordination failure in experiment 3 should significantly reduce both order variability and amplification,

compared to experiment 1, for two reasons. First, with greater confidence that others will use the optimal rule, there is less incentive to seek coordination stock, reducing the incidence and magnitude of deviations from equilibrium that may trigger instability. Hence,

Hypothesis 3a: *The provision of common information through the announcement of the optimal policy will decrease order variability.*

Second, even if such deviations occur, participants informed of the optimal policy should fully account for the supply line no matter what pattern of orders they receive. Hence,

Hypothesis 3b: *The provision of common information through the announcement of the optimal policy will eliminate supply line underweighting.*

Together with the auxiliary hypothesis of full rationality these hypotheses imply that knowledge of the optimal policy should allow the system to become more stable, with fewer oscillations and less amplification of orders up the chain.

5.2 Experimental Results

Figure 6 reports the results of experiment 3. Compared to experiment 1 (Figure 3), publicly providing the optimal policy significantly reduces the variability of orders, supporting H3a. The median standard deviation of orders falls to 6.2 (from 16.6 in experiment 1), a weakly significant difference ($p=0.084$). No outliers were detected, suggesting knowledge of the optimal policy prevented the extreme orders observed in some trials in experiment 1. Median total costs in experiment 3 are 3,396 and are weakly lower than in experiment 1 ($p = 0.099$), again supporting H3a.⁷ Announcing the optimal policy slightly reduces overall variability by reducing deviations from the equilibrium order of 4 cases/week; with less order variability, unintended inventory/backlog accumulation is smaller, reducing costs.

[Insert Figure 6 about here]

However, the results show the bullwhip effect is resilient. Orders and inventories still oscillate and there is significant amplification of orders up the supply chain ($\sigma_i > \sigma_{i-1}$ for 83% of the cases, $p = 0.0001$). Further, the tendency to underweight the supply line is not eliminated despite knowledge of the optimal policy. Examining the estimates of α and β in experiment 3, the median fraction of the inventory shortfall

⁷ Including the outlier teams from experiment 1 the median standard deviation of orders is 25.7 and the median total supply chain cost is 13,105. Both sets of differences now become strongly significant; ($p = 0.027$ for standard deviations and 0.032 for total costs).

corrected each period is 0.28, and the median fraction of the supply line participants consider is 0.09, both substantially below the optimal values of 1. In experiment 3, 82% of the estimated values of β are significantly less than one, 51% are not significantly different from zero, and zero is the best estimate of β for 21% of the participants. Estimates of α and β in experiments 1 and 3 are not statistically different (the 2-tailed Wilcoxon test yields $p = 0.11$ for α and $p = 0.81$ for β). Contrary to H3b, providing participants with the optimal policy does not reduce supply line underweighting.

5.3 Discussion

Experiment 3 reduces coordination risk by creating common knowledge of the optimal policy. Performance improves compared to the base case in experiment 1 without requiring additional costly inventory. Surprisingly, however, the improvement does not appear to come from a reduction in supply line underweighting. Instead individuals' initial orders remain closer to the optimal value of 4 cases/week, reducing the perturbations that move the system out of equilibrium and trigger the latent instability caused by their failure to account adequately for the supply line of unfilled orders.

6 Experiment 4: Eliminating Coordination Risk

Experiment 3 reduces but does not eliminate coordination risk because there is no guarantee that participants will use the publicly informed optimal policy. In experiment 4 we eliminate coordination risk completely by placing individuals in a supply chain with three automated players programmed to use the optimal policy. Participants are told the optimal, cost minimizing, decision rule as in Experiment 3, and are also informed that all other members of their supply chain have been programmed to follow this rule. As in experiments 1 and 3, the game begins in equilibrium, with constant and known demand of four, initial on-hand inventory of zero, and four units in each of the ordering and shipping positions. We ran 4 sets of 10 experiments with each set placing the human decision maker in a different role (i.e., 10 experiments with a human retailer, 10 with a human wholesaler, etc.).

6.1 Theoretical Predictions

If our explanation of the cause of instability and amplification in experiment 1 is correct, eliminating the possibility of coordination failure should eliminate (or at least significantly reduce) order variability and amplification. Overall supply chain performance will of course improve, since by definition the automated agents play optimally and experiments 1-3 show human players significantly deviate from optimal. The relevant comparison is how well the human player performs. Specifically, will human players deviate from the initial equilibrium in an attempt to build coordination stock? If they do, will they move to this new inventory level in an optimal fashion (essentially adapting an order-up-to policy with a

customized S' value) or continue to underweight the supply line?

Properly assessing these effects is complicated by the difference in the environment faced by the humans in experiment 4. Humans playing the upstream positions (W, D, or F) in this condition face no variability in incoming orders as all downstream players order the optimal 4 cases/week.⁸ They will not experience any unintended inventory changes, unlike their counterparts in experiments 1-3 who typically experience large excursions in incoming orders. In experiments 1-3 retailers are the only ones guaranteed to face constant orders. To control for these differences in variability and uncertainty we compare the behavior of the humans in experiment 4 against that of retailers in the other conditions. With the guarantee that all others will play optimally the human players should be much less likely to seek coordination stock due to failures of common knowledge, and they should be more likely to follow the optimal policy and fully account for the supply line. Hence,

Hypothesis 4a: *Eliminating coordination risk will decrease order variability for the human players relative to the retailers in experiment 1.*

Since we observed a decrease in overall variability in experiment 3 compared to 1 it is highly likely a similar reduction will be seen in experiment 4, where the automated optimal agents generate an even more stable environment for the human. However, players in experiment 3, despite common knowledge of the optimal rule, may still believe that their teammates would not use it. Experiment 4 eliminates this failure of trust in others' actions and also ensures that there is no variability in incoming orders. Hence,

Hypothesis 4b: *Eliminating coordination risk will decrease order variability for the human players relative to the retailers in experiment 3.*

Finally, note that in experiment 4 the sole human player is the only source of change: inventory can only change if the player deviates from the optimal order of 4 cases/week. There is no reason to do so, but, freed of the need to devote scarce cognitive resources to anticipating how their teammates will behave, participants in experiment 4 should be more likely to use the optimal policy and fully account for the supply line if they do choose to deviate from equilibrium. This leads to our final hypothesis,

⁸ While the automated agents are guaranteed to play the optimal strategy, their orders may differ from 4 cases/week if the human player does not order optimally. If the human deviates from equilibrium (for any reason), upstream agents experience unintended changes in their inventory, requiring them to alter their own orders to correct the imbalance (using the optimal order-up-to rule to do so).

Hypothesis 5: *Eliminating coordination risk will eliminate supply chain underweighting.*

6.2 Experimental Results

Figure 7 depicts the average order standard deviation for the human participants in experiment 4.

[Insert Figure 7 about here]

Order oscillations for the entire team are dramatically reduced relative to experiment 1 (note the scale differences between figures 3 and 7). The median standard deviation of orders is now 1.5 compared with 3.9 for retailers in experiment 1, a significant difference (one-tailed Wilcoxon test, $p=0.016$). The median cost for a single role in experiment 4 is 47 which is significantly lower than the median retailer cost in experiment 1 of 336, ($p = 0.0039$). Both comparisons support hypothesis 4a.⁹

Comparing experiments 3 and 4, the median standard deviation for retailers in experiment 3 is 2.4, which is not significantly higher than the median standard deviation of 1.5 in experiment 4 ($p = 0.16$). Median retailer cost in experiment 3 is 122, significantly higher than median costs of 47 in experiment 4, ($p = 0.048$).¹⁰ The data do not consistently support Hypothesis 4b, although variability decreases (but not significantly so) and the cost difference provides some support for the hypothesis.

Lastly, the estimates of α and β in experiment 4 are similar to those in the other conditions. The median fraction of the inventory shortfall corrected each period is 0.31, and the median fraction of the supply line participants consider is 0.24, both substantially below the optimal values of 1, as in the other experiments. Hypothesis 5 is not supported. Supply line underweighting remains robust despite knowledge of the optimal ordering rule and the elimination of coordination risk.¹¹

6.3 Discussion

Experiment 4 eliminated coordination risk entirely by automating all but one supply chain role, guaranteeing that the human player's teammates always make optimal decisions. The variability of orders

⁹ Including outliers the median standard deviation of orders placed by retailers in experiment 1 is 3.86, and by all players in experiment 4 is 1.6. The median retailer cost in experiment 1 is 279 and the median cost in experiment 4 is 54. Both differences are highly significant ($p = 0.011$ for σ and 0.0074 for costs).

¹⁰ Including outliers, the difference in the standard deviation of orders between experiments 3 and 4 is not statistically significant ($p = 0.14$); the difference in costs is weakly significant ($p = 0.056$ for costs).

¹¹ Estimates of α and β in experiments 1 and 4 are not statistically different (2-tailed Wilcoxon test, $p = 0.35$ for α and 0.30 for β). For α , 97% of the estimates are significantly less than the optimal value of one. For β , 93% of the estimates are significantly less than the optimal value of one.

placed by the human in experiment 4 is significantly lower than the variability of retailer orders in experiment 1, but not significantly lower than that of retailer orders in experiment 3. Failure to use the optimal decision rule and lack of common knowledge appear to be the main sources of the bullwhip effect in the experiments, while lack of trust in others' actions does not significantly affect behavior.

7 Discussion and Conclusion

Table 2 summarizes results by presenting the median and average standard deviations of orders placed and RMS deviations from the optimal steady-state orders, with and without outlier teams, as well as the average costs, in each of our four experiments, showing which differences are significant.

[Insert Table 2 about here]

Our analysis contains four main findings.

1. We find that the “bullwhip effect” persists with constant and known demand—the bullwhip effect in supply chains is in part a behavioral phenomenon.
2. We identify *coordination risk* as a new source of uncertainty that may cause suboptimal decisions and trigger the latent instability and amplification caused by poor decision rules that do not adequately account for the supply line of unfilled orders and time delays in the system.
3. Performance is improved through the introduction of *coordination stock*. In contrast to traditional safety stock, used to buffer against exogenous variations in customer demand, coordination stock buffers against endogenous deviations from optimal behavior made by supply chain partners.
4. Publicly explaining the cost-minimizing decision rule improves performance without requiring additional inventory. However, providing further assurance that all team members will follow this rule does not yield significant additional improvement.

All research has limitations and ours is no exception. These limitations provide directions for future research. We used students as participants, as is common practice in experimental economics. It is possible that experienced supply chain professionals would have performed better, although at this time there is no evidence of any systematic differences due to the subject pool. For example, Croson and Donohue (2002) report results from supply chain managers and find similar patterns of bullwhip and underweighting of the supply line. Using professionals as participants would further test the external validity of these results.

The specific choice of initial conditions and parameters suggests other extensions. Although there is no *a priori* reason to think so, it is possible that different cost parameters or levels for initial inventory might lead to different outcomes.

Finally, experiment 4 provides an initial starting point for investigating how people interact with agents programmed to follow specific decision rules. While our agents were programmed to follow the cost-minimizing decision rule, further research could explore participants' responses to agents programmed to use other rules, including suboptimal rules and the rules estimated from the human players.

Our work suggests three managerial insights. First, we demonstrate that the bullwhip effect is a behavioral phenomenon as well as an operational one, and therefore methods for reducing supply chain instability should address the behavioral as well as structural causes of the problem. Decision makers have a difficult time controlling systems that include feedbacks and delays, so training and decision aids that help them do so may lead to substantial improvement. Similarly, the notion of "optimal" behavior is contingent on people's assumptions about the thinking and behavior of the other agents with whom they interact. The results show that people's mental models of the structure and dynamics are systematically suboptimal, and that many believe their counterparts behave in an unpredictable and capricious fashion. In contrast, most economic models of supply chains assume agents are rational and will play the optimal strategy. The failure of this assumption may explain both the popularity of policies that force centralized controls on supply chains and the strong resistance to their implementation on the part of many managers.

Second, we identify a new behavioral contributor to the supply chain instability—coordination risk. To the extent that managers can be assured of their supply chain partners' knowledge of the optimal decision rule and trust them to implement it, performance can improve further. Our final contribution is the preliminary identification of simple means to alleviate the bullwhip effect in supply chains. We test three mechanisms in the laboratory—ordering automation, coordination stock and common knowledge—and find that all three improve performance to some extent. The fact that these mechanisms work in the laboratory does not guarantee that they will be effective in practice, but it does signal a fruitful direction for future experimental and empirical research.

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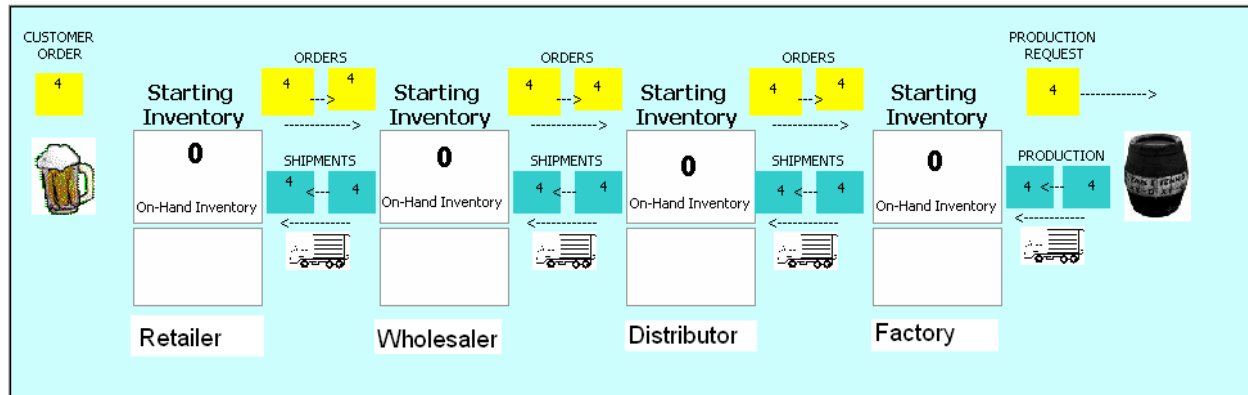


Figure 1: The Beer Distribution Game portrays a serial supply chain.

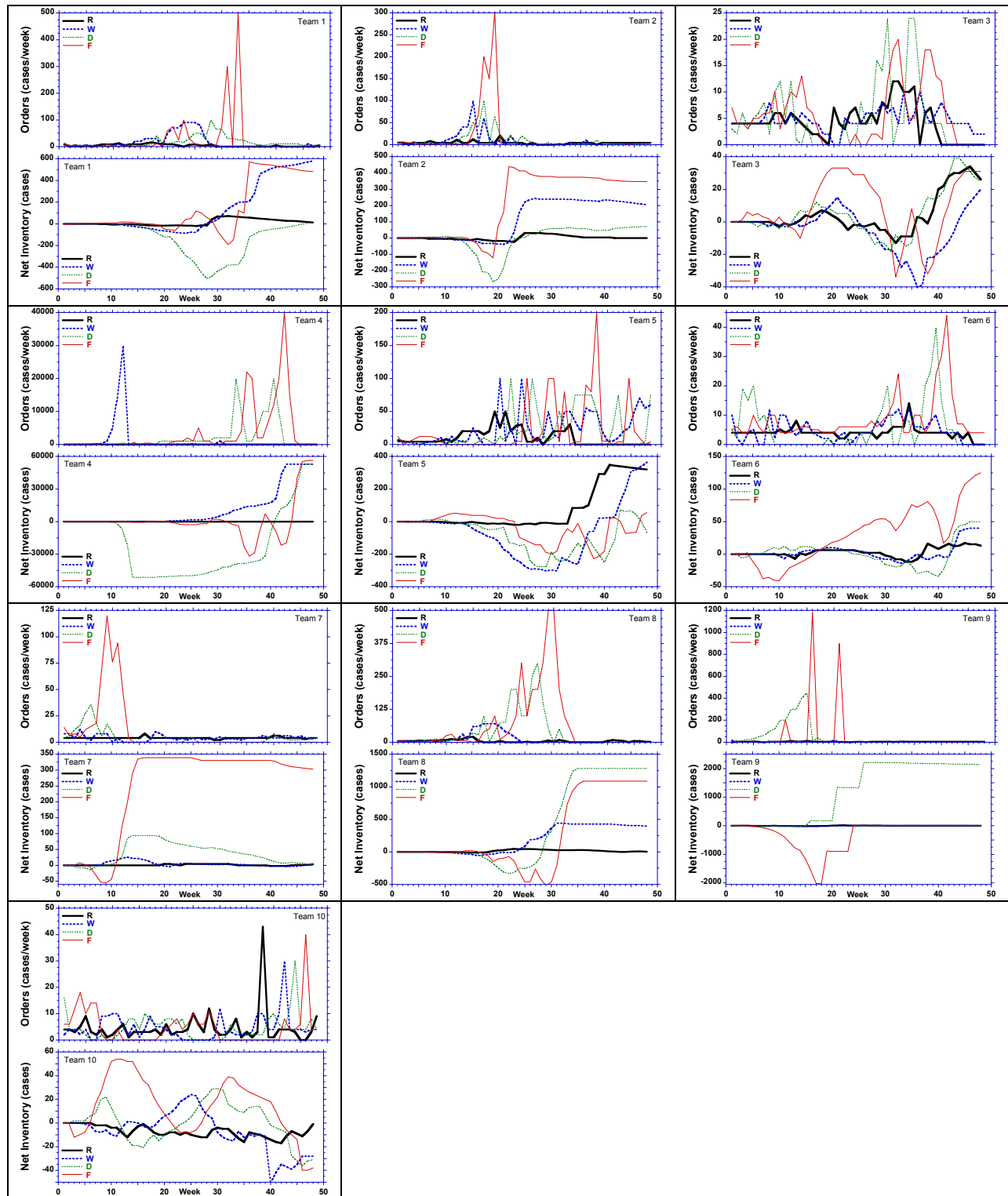


Figure 2. Orders (top) and On-Hand Inventory (bottom) for the 10 teams in Experiment 1 (baseline).

Negative inventory values indicate backlogs. Note: vertical scales differ across teams.



Figure 3: Standard Deviation of Orders in Experiment 1 (baseline).

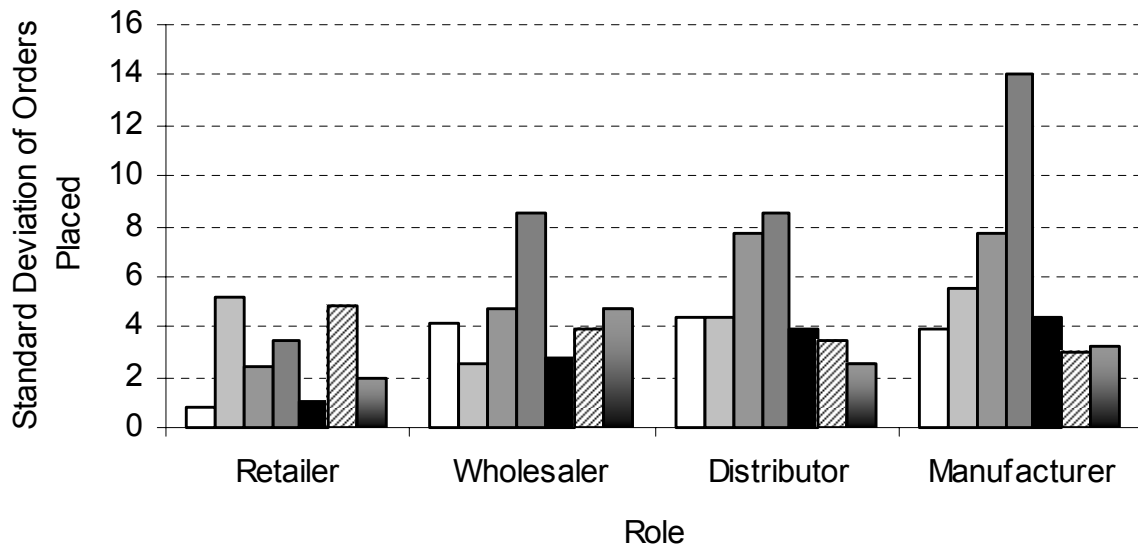
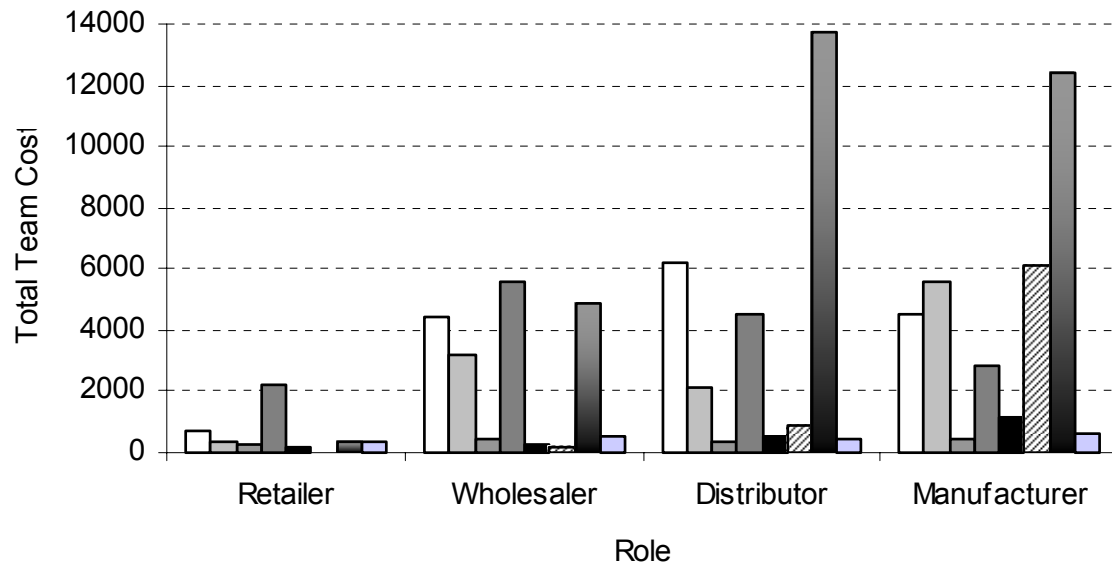
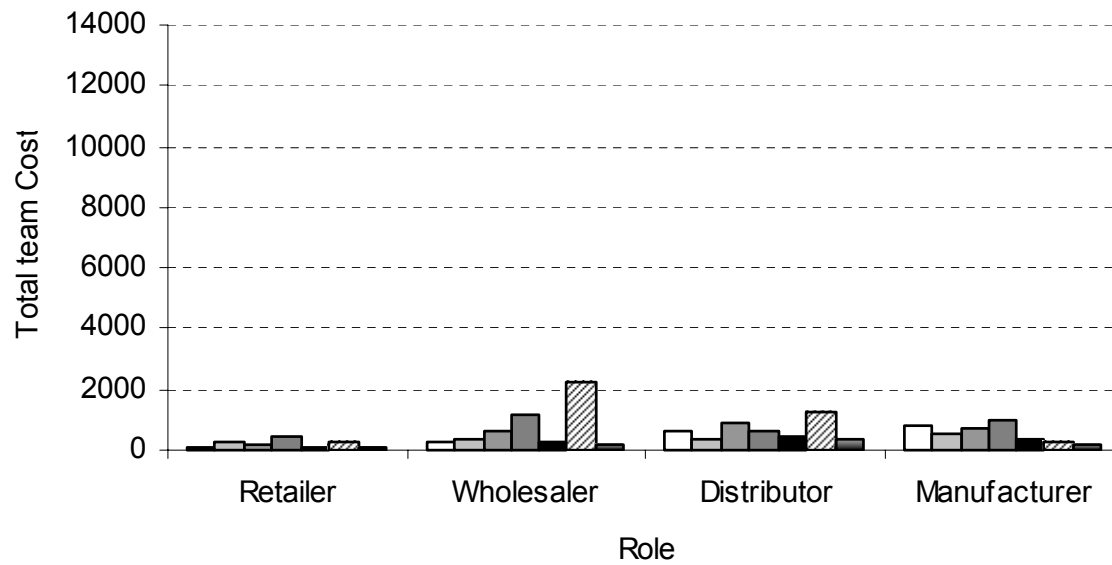


Figure 4: Standard Deviation of Orders in Experiment 2 (adding coordination stock).



(a) Experiment 1 (baseline)



(b) Experiment 2 (adding coordination stock)

Figure 5: Supply Chain Cost for Experiments 1 and 2.



Figure 6: Order Standard Deviations for Experiment 3 (adding common knowledge).

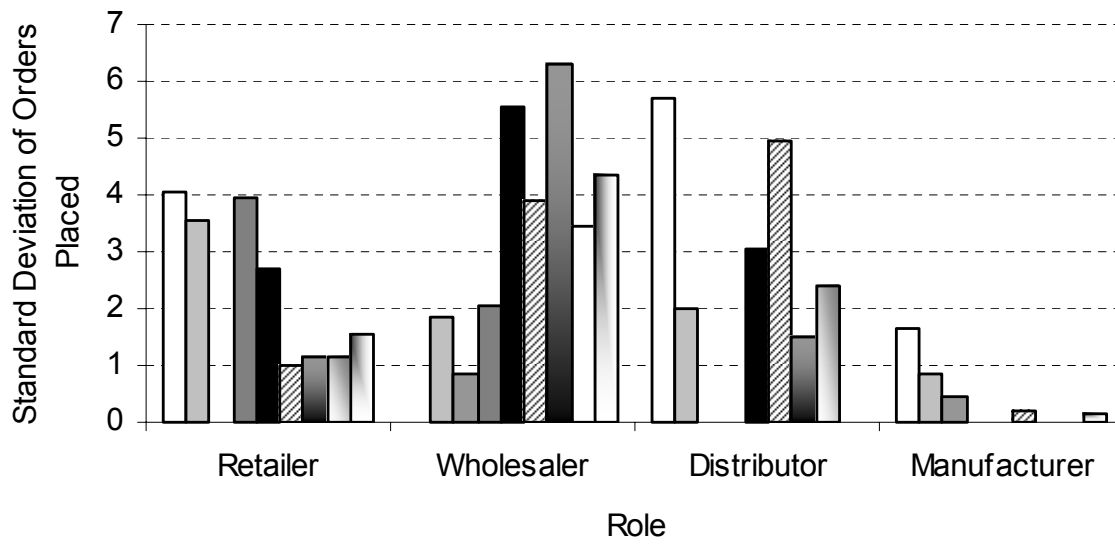


Figure 7: Order Standard Deviations for Experiment 4 (eliminating coordination risk).

	θ	α	β	S'	R^2	RMSE
Experiment 1: (Initial inventory = 0; no common knowledge of optimal policy)						
Median Estimate	0.17	0.43	0.10	4.69	0.51	6.14
Median Width of 95% Confidence Interval	0.89	0.32	0.34	14.96		
N	40	40	37	37		
Experiment 2: (Initial inventory = 12; no common knowledge of optimal policy)						
Median Estimate	0.17	0.26	0.26	6.04	0.52	2.85
Median Width of 95% Confidence Interval	0.63	0.26	0.45	11.68		
N	40	40	36	36		
Experiment 3: (Initial inventory = 0; common knowledge of optimal policy)						
Median Estimate	0.21	0.28	0.09	8.26	0.57	2.83
Median Width of 95% Confidence Interval	0.70	0.22	0.37	10.90		
N	40	40	39	39		
Experiment 4: (Initial inventory = 0; common knowledge of optimal policy and automated, optimal agents)						
Median Estimate	0.46	0.31	0.24	3.43	0.28	1.18
Median Width of 95% Confidence Interval	0.87	0.29	0.56	7.77		
N	40	40	37	37		
Median Estimates, Sterman (1989a)						
	0.25	0.28	0.30	15	0.76	2.60
N	44	44	40	40		
Median Estimates, Croson and Donohue (2002)*						
	N/A	0.22	0.14	N/A	0.71	
N		44	44		44	

Table 1: Estimated parameters for the ordering decision rule (eq. 1).

Note that the number N of estimates for θ and α is larger than for β and S' because for some participants the estimate of $\alpha = 0$, in which case β and S' are undefined.

* Croson and Donohue (2002) estimated a slightly different ordering decision rule:

$$O_{i,t} = \text{Max}[0, a_i S_{i,t} + b_i SL_{i,t} + c_i IO_{i,t} + d_i + \varepsilon_{i,t}]$$

where IO is incoming orders and a, b, c, d are estimated. The inventory and supply line weights α and β can be expressed in terms of a and b by interpreting cIO as the participant's forecast of demand and noting that the inventory correction term $d + aS + bSL = a[(d/a) - (S + (b/a)SL)]$, which implies $\alpha = -a$ and $\beta = b/a$.

Measures of Orders Placed	Experiment 1: Constant Demand (Retailers only)	Experiment 2: Coordination Stock	Experiment 3: Common Knowledge (Retailers only)	Experiment 4: No Coordination Risk	Significance tests: ** p < 0.05 * p < 0.10
Outlier teams excluded					
Median std. deviation	16.6 (Retailers =3.9)	3.8	6.2 (Retailers =2.4)	1.5	1 > 2** 1 > 3*1 > 4** 2 = 3 3 = 4
Average std deviation	20.6 (Retailers = 4.7)	6.8	8.2 (Retailers =5.5)	2.1	
Median RMS deviation	16.0 (Retailers =3.6)	4.2	6.4	1.5	1 > 2** 1 > 3* 1 > 4** 2 < 3**3 = 4
Average RMS deviation	22.0 (Retailers =4.8)	4.6	8.6	2.2	
Median Total Supply Chain Cost	9,151 (Retailers =336)	1,890	3,396 (Retailers =122)	47	1 > 2** 1 > 3* 1 > 4** 2 < 3*3 > 4**
Average Total Supply Chain Cost	10,771 (Retailers =546)	2,157	4,562 (Retailers =202)	147	
Outlier teams included					
Median std deviation	25.7 (Retailers =3.9)	5.0	6.2 (Retailers =2.4)	1.6	1 > 2** 1 > 3** 1 > 4** 2 = 33 = 4
Average std deviation	455.1 (Retailers =4.5)	57.8	8.2 (Retailers =5.4)	2.3	
Median RMS deviation	27.8 (Retailers =3.8)	5.0	6.4 (Retailers =2.4)	1.6	1 > 2** 1 > 3** 1 > 4** 2 = 33 = 4
Average RMS deviation	485.4 (Retailers =4.6)	63.2	8.6 (Retailers =5.7)	2.3	
Median Total Supply Chain Cost	13,105 (Retailers =279)	2,840	3,396 Retailers = (122)	54	1 > 2* 1 > 3** 1 > 4** 2 = 3*3 > 4*
Average Total Supply Chain Cost	202,646 (Retailers =463)	14,101	4,562 Retailers = (202)	159	

Table 2: Supply chain costs and metrics of order amplification (RMS deviation is the root mean square deviation of participant orders from the equilibrium value of 4 cases/week). The figures in parenthesis for experiments 1 and 3 are for retailers only, and are used to compare with experiment 4.

Appendices

A-1. Payment Scheme: We used the following payment scheme to reward players for their team's performance. First, each participant received \$5 for showing up to the experiment. In addition to their show-up-fee, participants in the best-performing supply chain earned an extra \$20, while participants in the worst performing supply chain earned no extra money. Performance was measured by the total cost incurred by the four roles over the course of the game. Participants in the middle-ranking supply chains earned bonuses according to:

$$\text{bonus} = \$20 \frac{(\text{chain profit}) - (\text{worst performing chain profit})}{(\text{best performing chain profit}) - (\text{worst performing chain profit})}$$

This payment scheme has a number of attractive properties. First, it provides a continuous incentive for participants to earn profit in the game. In contrast to previous experimental implementations in which the highest-earning group won a fixed prize (e.g. Sterman 1989a), each group has an incentive to increase their profits, even if they cannot win first place by doing so. Second, the design discourages collusion among participants to artificially raise the profits of all teams together. Third, the payment represents the benchmarked performance of an integrated supply chain “firm,” a performance metric often used in industry.

A-2 Outlier Analysis: To test for outliers, we used Grubb's procedure, or maximum normed residual test, (Grubbs 1969) as recommended in the Engineering Statistics Handbook (National Institute of Standards and Technology; www.nist.gov). The procedure involves calculating the standard deviation of orders placed over the entire game for each individual and comparing these standard deviations to those of others in the same role/experiment category (e.g., all retailers within experiment 1). The Grubbs procedure then identifies outliers within each role or experimental condition. For experiment 4, with one human and three automated team members, a team was eliminated if its single human subject did not pass the criterion. For the all-human experiments (experiments 1-3), a team was eliminated if two or more of its members did not pass the criterion. Table A.1 lists the individuals identified as outliers and the number of teams eliminated as a result, for each experiment.

	Experiment 1: Constant Demand	Experiment 2: Coordination Risk	Experiment 3: Coordination Stock	Experiment 4: Common Knowledge
Outliers by Role (team #)	Retailer (5) Wholesaler (4) Distributor (4) Manufacturer (4) Distributor (9) Manufacturer (9)	Retailer (18) Wholesaler (27) Manufacturer (1) Manufacturer (5)	Distributor (6) Manufacturer (6) Retailer (7) Wholesaler (7) Distributor (7) Manufacturer (7) Retailer (10) Wholesaler (10)	Retailer (8) Distributor (7)
Eliminated Teams	Teams 4, 9	Teams 1, 5, 18, 27	Teams 6, 7, 10	None

Table A.1. Individual Outliers and Eliminated Teams

The direction and statistical significance of our results is not changed by the removal of these outliers. Table 2 includes results with the outliers identified above included.